

# Canadian Journal of Psychology

THE JOURNAL OF THE CANADIAN PSYCHOLOGICAL ASSOCIATION

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# Canadian Journal of Psychology

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## EDITORIAL ANNOUNCEMENTS

### RESIGNATION OF DR. JOHN A. LONG

AT THE Annual Meeting in May the Publications Committee announced that it had regretfully accepted the resignation of Dr. John A. Long as Editor of this journal. Dr. Long's unanimous election as Chairman of the Board of Education of Toronto's newly-created Metropolitan Area, added to his full-time duties as Director of Educational Research at the Ontario College of Education, forced this step upon him.

As editor of the *Canadian Psychological Bulletin* from 1942 to 1947, and of the *Journal* since that date, Dr. Long has shown judgment, skill, and devotion to his task which have earned him the profound thanks of the whole Association. The present valid and secure position of the *Journal* is largely his achievement, and we wish him every success in the future.

Dr. Long has frequently stated that he could never have carried on without the tireless enthusiasm and efficiency of his Assistant Editor, Miss K. M. Hobday, and we are happy to say that she has consented to remain at her post during the difficult period of transition.

To acknowledge our great indebtedness to the retiring editor is also to measure the calibre of the person needed to succeed him. The Directors and this Committee announce with pleasure that they have found a worthy successor in Professor J. D. Ketchum, of the Department of Psychology, University of Toronto. His literary gifts and grasp of contemporary psychology promise an era of publication which will fully complement the distinguished work of his predecessor.

No editor, however, can create a journal single-handed, and we ask all members of the CPA to give Professor Ketchum the utmost co-operation in the difficult and important task which he has undertaken for us.

D. C. WILLIAMS  
*Chairman, Publications Committee*

## FROM THE NEW EDITOR

AT THE START of this unexpected and formidable new task, all I can do is express my willingness to give my best to it, and my hope that I may not fall too far below the standard set by my predecessor.

In uttering this hope I am, of course, aware that my part in making the *Journal* worth reading will be only a modest one. I have already had reason to recognize how indispensable is the aid of an experienced assistant such as Miss Hobday, and how heavily we are in debt to our publishers, the University of Toronto Press, for their generous editorial, technical, and financial help.

Ultimately, however, the standard of the *Journal* depends on those members of our Association who prepare and submit articles. Our aim is to reflect as fully as possible the activities and interests of Canadian psychologists, whether in research, in psychological theory, or in the teaching and applications of psychology. Success in achieving that aim lies in other hands than ours—in the hands that commit these activities to paper and mail the paper to the Editor. The flow of manuscripts has of late been encouraging; as it continues to increase, the standard of publication will rise, and we may at length attain that luxury of the well-to-do—a clear and distinctive editorial policy.

In the meantime we will do our best to produce a worthy journal, and we ask the forbearance of readers and contributors for any editorial shortcomings. These could be considerably reduced by the careful preparation of manuscripts, and a few suggestions on this point will be found on page 144.

Articles are always welcome, and so are comments and criticism from our readers. Publication of the *Journal* is the largest single undertaking of our Association, and also the most expensive; we feel sure that it is also the one in which our members are most eager to share and to help.

J. D. KETCHUM

## ON HUMAN THOUGHT<sup>1</sup>

D. O. HEBB  
*McGill University*

IN THE TOPIC of human thought I have chosen what is indubitably the central problem of psychology—at once the most difficult and the most important that scientists face. Its difficulty is the burden of my address; as for its importance, this can be seen both in the technical field of research and in the practical world of affairs.

Thorndike tried to dispose of the difficulty fifty-five years ago by denying its existence. He buried thought in 1898, but the ghost insists on walking. Often we decline to say that animals think; but comparative psychology has been unable to avoid concluding that animals have expectancies, insights, hypotheses, conceptual activities, a variable attention, and so forth. These are but aspects of thought; and if we cannot deal with the comparatively simple behaviour of animals without taking account of thought, how adequate can a thought-less human psychology be?

Again: no psychologist will need to be persuaded of the urgency of dealing more effectively with social behaviour. Nuclear fission would be simply a splendid triumph of physical science if only we had solved the problem of social relations. If behaviour is—as it must be—directly determined by what goes on within the skull, only indirectly by sense stimulation and bodily need, you will agree that the problem of social relations is the problem of what one man *thinks* of another, of the *attitudes* of fear and suspicion and greed for power that make war possible, or alternatively the attitudes of generosity and charity (in the true sense of love) that show man how to live with his fellows.

This is of course not our problem alone. It would be arrogant to suggest that this is solely a problem for the technically trained psychologist. Teacher, priest, politician, and novelist, newspaper writer, and dramatist, the socially perceptive man in whatever occupation, these are all psychologists too, in a broad sense of applied psychology. Moreover, it is they rather than the technically trained psychologist who have broken trail so far, and who must continue to do so much of the time in the day-to-day solution of our social problems. Perhaps I may remind you of the parallel case of physics and engineering. Only in quite recent times has physics made any significant contribution to man's practical problems, and one may recall the remark that the steam engine did more

<sup>1</sup>Presidential address, delivered at the Annual Meeting of the Canadian Psychological Association, Kingston, May 29, 1953. The critical assistance of Beatrice Syme is gratefully acknowledged.

for physics than physics ever did for the steam engine. So with psychology and the practical engineers of society. But psychology has just about come of age, and is already making clearly significant contributions to social problems. Certainly there is evidence enough now to show that we are justified in confidently looking forward to the day, not too far in the future, when psychology will bear to social education and planning the same relation that the physical sciences now bear to the technology of engineering.

Some of you may feel that psychology is already prepared for that role. You may feel that the next step is simply to put psychologists in charge, to recognize us as the authorities wherever human behaviour is concerned. I cannot share this view, for two reasons. One is that politics, or equally education, is an art and not a science. A psychologist, *qua* psychologist, is not a politician or teacher for the same reason that the physicist is not an engineer, or the biochemist a physician.

The second reason is that we have as yet made no more than the barest beginning at understanding human behaviour, and must still maintain a proper modesty in counselling on practical issues. But do not mistake me: I am not selling psychology short; our knowledge of behaviour is growing at an unprecedented rate; there is already a significant increase in the rate of practical applications, and the new vistas that are opening up promise one day to give psychological knowledge a degree of practical significance that we may not even dream of now.

#### THE RELATION OF THEORY TO UNDERSTANDING

So much by way of introduction—or, if you like, I have given you my conclusions first, and propose now to consider the reasons therefor. My aim tonight is to take a look at the theory of thought, to see where it has got to; but also to look at the vast *terra incognita* of the subject, awareness of which must keep us modest in our claims to understanding. First let me distinguish between theory and understanding.

In developing theory, one must simplify and schematize, select the phenomena to be dealt with, and in general shut one's eyes to the whole rich complexity of the real world. The more complex the phenomena the more necessary this becomes. The more complex the phenomena, therefore, the greater the gap between theory and fact in the earlier stages of development. I propose to you that scientific understanding always involves two things. First, it means having a fully intelligible (and thus unreal) model or scheme or *theory* of the phenomena to be understood, lacking the detail and apparent contradictions of reality. Secondly, understanding means seeing the incompleteness and defects of the theory. In short, scientific understanding is the impact of theory on

perceived reality. Without theory, no understanding; with theory alone, a defective and sterile understanding—if any at all.

It is not always easy to keep these two aspects of understanding in mind at the same time. Yet we must do so. For theory, oversimplification is necessary and desirable, but if the larger reality is not kept in mind, if one is not constantly mindful of the discrepancies from fact, theory becomes static and sterile. For practice, the discrepancies must be emphasized, and yet theory in some form is the only source of order in dealing with facts.

It should not be necessary to point out these two aspects of understanding, but I fear it is. In psychology we have workers who are blinded by theory—having rightly simplified, they have wrongly forgotten the phenomena that were set aside temporarily. But we also have those who are over-impressed by the shortcomings of theory. They fall into the equally sterile posture of the hypercritical. These are the psychologists who can accept no theory that is not perfect, the ones who either do not experiment at all because they are waiting for the perfect experiment or experiment only to show that existing theory is no good.

If it is true that understanding means, first, thinking in some sort of analogy, and secondly, seeing where the analogy (or model) falls short, this leads to the conclusion that the main line of descent of psychology is through Hobbes and Locke and the Mills, to Wundt and Titchener; and later, in a closely related line, Thorndike, Watson, Holt, and Hull, through all of whose work runs the attempt somehow to make all thought a relatively simple matter of learning, to make it *intelligible* by the mechanisms of association. These men were narrow; they were wrong; and without them, without the simplification they achieved, modern psychology would not exist.

If you believe with me that Gestalt psychology has offered us a far more satisfying account of human activity, I must point out that Gestalt psychology hardly exists except as a series of reactions against one aspect or another of associationism. Up to 1935, at least, there was no unified and consistent statement of the views of the group—and even Koffka's book of that year offered little more than the appearance of system, a verbal rather than a true synthesis of the discordant undertakings of the Gestalt group.

The conception of traces, in Gestalt hands, is open to all the powerful criticism that Koffka and Köhler had made of Watson's synaptic resistances, and is fully incompatible with their treatment of perception. The theory of perception was never united effectively with the theory of thought; and, I think, for a very good reason: the two are again incompatible. The isomorphic approach to perception makes it stimulus-bound,

whereas the very essence of Köhler's treatment of insight and thought is the denial that behaviour is dominated by the situation.

But the Gestalt pearls still hold together on the thread of opposition to the associationists and conditioned-reflexers. Gestalt psychology is not theory, but a running battle against theory. We must, therefore, credit the narrowness of Wundt and Titchener, and of Thorndike, Watson, and Holt, with the rich products of revolt against their narrowness, in the brilliant work of Külpe, Hobhouse, Wertheimer, Köhler, Lashley, Tolman, and Lewin. All of this latter group are far better understood as rebels than as theorists.

So, it appears, we find the genealogy of modern psychology, and its *raison d'être*, in one form or another of learning or association theory. In saying so, I leave to one side psychoanalytic theory, which would take too long to go into here, only remarking parenthetically that its failure to develop more satisfactorily may be related to its relative disregard of learning and the mechanisms of association.

For academic psychology, at least, learning theory is at the heart of things: wrong, if you like, narrow, rigid, unpleasing—*indigestible*—but the trunk of the tree that bears the fruit of more palatable ideas. If you like roses, don't abolish rosebushes. I learned as a student to scoff at Thorndike's imperceptiveness in his first studies of animal intelligence in 1898, and his conclusion that animal learning is simply "trial and error and accidental success." I begin to see, however, that Thorndike did not merely provide the incentive for the more adequate experiments of Hobhouse and Köhler: without Thorndike, I doubt now that Hobhouse could have seen how to do what he did. Some things are simple in retrospect, but that does not mean that they were easy to see in prospect, and I am inclined now to rate Thorndike's *tour de force* above Hobhouse's, beautiful as Hobhouse's experiments were.

#### THE SCHEMA OF ASSEMBLY AND PHASE SEQUENCE

You may rightly regard some of this as being also an *apologia* for my own theorizing, as reported in a book that our host Professor Blackburn and I have agreed to consider the product of my own perverted taste for science fiction. Let us briefly<sup>2</sup> look at some of its properties as a schema or model, especially in the light of Humphrey's *Thinking*. We are back to some of the problems of 1910 in current psychology; but with heavier artillery from modern physiology, and more accurate range-finders from modern studies of behaviour. The insight that illumines Humphrey's synthesis of 19th- and 20th-century psychology has a particular relevance and value today.

<sup>2</sup>This part of the address has been condensed for publication. It will be presented in full elsewhere.



On page 26 he summarizes the criticisms of association theory, and suggests that these difficulties are so persistent, in such different contexts—from Wundt to Watson—that they may be “inherent in the nature of an associational theory of any kind.” On this two comments may be made. I have already proposed that there is no theory but the associational, in one form or another; other approaches perhaps offer more interesting experiments, propound the problems better, or are more stimulating, but they have not been productive of a systematic explanation. If this is so, the criticisms are not of associationism as such, but refer to the difficulties encountered so far in all attempts at systematic psychological explanation.

Secondly, the theories Humphrey deals with have in common certain tacit preconceptions about neural function that are now outmoded. It is here rather than in the conception of association that the difficulties originate. All older theories were sense-dominated, an approach which is no longer possible unless one shuts one's eyes to the existence of such work as that of Berger, Adrian, Lorente de Nó, and Morison and Dempsey.

Some of the classical criticisms concern the directedness and creativeness of thought. As long as the only source of organization lies in the sequence of sensory events, past or present, this is a decisive refutation of association theory. But neurophysiology not only permits, it requires the assumption that the activity of the brain is not under complete sensory control, and this radically changes the theoretical problem. If there is, for example, a continuing set of neural activities, not sensorily limited, which constitutes an *expectancy* of some goal or other, the facilitation from this may act as a guiding agent on the other central processes that are in direct control of behaviour, selectively supporting the response to some aspects of the environment and not others. The animal, then, is no longer an aimless responder to environmental stimulation but would show the rudiments of purpose and direction.

The assumption of sense dominance in older theories left no possibility of a higher-order activity in parallel which could (a) “guide” the series of events directly controlling behaviour, by supporting this response and not that, and (b) lead to new fusions, new combinations of old elements that would constitute insight and creativity. The conception of the train of thought not as a single series but as existing in multiple-parallel has some very interesting explanatory possibilities. For example, W. R. Thompson and I have recently tried to show that it permits a considerable clarification of the social phenomena of empathy, co-operation, and communication.

Another major criticism of older theories is that items of thought are not images or reproductions of specific sense-events. In *The Organization*

of *Behavior* I emphasized the case in which an assembly (in my model, the unit of thought) corresponds to a rather specific sense event. But this was for the sake of clear communication, and I pointed out briefly (pp. 105-106, footnote p. 47) other possibilities, which I would now make more central in the theorizing. It was shown (pp. 43-44), for example, how the perception of relative brightness might be accounted for. There is reason to think that looking from *any* moderately bright surface to a brighter (or darker) one could make a particular group of cortical cells active. If these become organized in an assembly, the activity of this assembly would be the basis of a relative perception, *with no specific sensory content*. Such an item in thought would be exactly the sort of thing demanded of association theory by Bradley in 1883.

I realize that all this skates rapidly over some very difficult matters. I would not leave the impression that these problems are now solved. They are not, as we well know. We can claim for such ideas, however, that they free us from the tyranny of older ones. In a sense, we now have too many new explanatory possibilities to choose from; assuming that only one is "right," our new freedom is embarrassing to the theorist. His chances of being wrong are multiplied. But our psychological problems are clearly changed, and it is no longer true—even remotely—that the primary effect of physiological ideas in psychology is limiting and stultifying. On the contrary, they present a new stimulus, a challenge to creative thought that as scientists we cannot refuse.

#### THE PHENOMENA OF HUMAN THOUGHT

So much for the more technical side. It remains to look at the second half of the problem: its broader aspects, which are not yet comprehended within the model, not yet explained by theory, but which theory, if it is any good, will help us to see—when we look for broader aspects. I have already referred to the danger that one may forget the facts that one's theory does not comprehend. A more general forgetfulness concerns those phenomena that are not easily brought into the laboratory, and this I believe applies particularly to human thought.

Consider first the study of problem-solving. We know this phenomenon in two contexts: first, as it has been studied in the laboratory; secondly, as we know it ourselves in our daily lives as scientists and citizens. One of these bears almost no relation to the other, but one would not guess it from the psychological literature. Why? Because it would be extraordinarily difficult to get the second form of problem-solving into the laboratory so that it could be studied experimentally.



It is notorious that a chief difficulty, almost the essence of real-life problem-solving, is to find out what the problem is, to learn that there is a problem, or to determine which of our many problems have feasible solutions. Once scientists at large knew that an atomic bomb had been used, it was inevitable that the Russians would make one; for the knowledge that it was now possible was a great part of the problem-solving process. Espionage only speeded things up. But when we study problem-solving in the laboratory the first thing we do is tell the subject what the problem is and thus miss the essence of the matter.

We cannot easily bring a freshman into the laboratory and simply say to him, "There is a problem around here somewhere for you to find and solve"; though a little ingenuity, with the use of a number of such undefined problems, might permit some approximation to these conditions. Still less can we bring the freshman in without even telling him the problem is there, day after day, until he happens on it and solves it.

To study this, the most important aspect of human problem-solving, we must, I think, depend mainly on the fossil record of scientific thought, embedded in the stony strata of our libraries and in the histories of science. Here will appear again and again the illogicality of the scientist's thinking, the heavy dominance of tradition, the bright faith that kept perhaps generations of thinkers working on at the insoluble problem until it was finally solved. Here too it will appear that the true difficulty is in asking the proper questions, in getting ideas clarified, in finding the fruitful way of looking at and arranging one's data. To achieve the notion of torque, for example, took 200 years or so of painful work by a series of men of the greatest ability; once clear, it could be communicated in five minutes to any student of ordinary intelligence.

I am tempted to go on with this topic, for the history of scientific thought offers a rich field for the student of human intelligence, once he frees himself of the notion that it has much to do with logic as the logicians describe it. But this would be a by-way, for we are trying now to get a more comprehensive view of thought and must not restrict ourselves to the *relatively* rational and *relatively* systematic problem-solving of the scientist—or, for that matter, of the business man, or the inventor or mechanic or advertiser who is aware of having a problem and self-consciously sets out to find an answer. What are the characteristics of thought in other circumstances, in our social life, in art appreciation, in our amusements, our prejudices, jokes, jealousies, and fears?

Obviously, I have not left myself time to deal with all this. I know very well, too, that I have not the insight and understanding to draw

this picture complete; the development of such understanding will come only as our inadequate models of man's thought become more adequate. Understanding is not restricted to the models, of course, but it still stays within hailing distance, and can only advance as they advance.

However, the model I have described extends our purview somewhat, and I shall take two examples that will show, I think, how far we are from explaining man's thought, and—if I may say so, parenthetically—what risks are run by the psychologist who dogmatizes, on the basis of current theory, about human behaviour. One of my examples concerns the structure of society; the other, what I have called the psychology of hocus-pocus.

Thompson and I have recently taken a fresh look at human social behaviour, considering it from the point of view of comparative psychology. As a guide in this undertaking, we availed ourselves of an observation of President Lowell of Harvard. As he introduced the eminent entomologist W. M. Wheeler for an honorary degree, Lowell remarked that Wheeler had shown that ants, too, build a complex society without using intelligence. We asked ourselves: what mental mechanisms determine the form of our social organization? It is quite clear that this organization completely depends on our intellectual capacities; the question is, do we differ from the ant, and from the gregarious birds and mammals, *solely* in intellectual capacity?

The answer, according both to Lowell and to Thompson and me, is emphatically no. Certain features of the model or theory already described suggested that man might be as distinctive emotionally as he is intellectually; and when we looked at human society with this in mind we found a great deal to suggest that society is fundamentally a function of man's emotional susceptibilities. Our social arrangements are primarily a sort of protective cocoon for the fragile mechanisms of human thought. For our present purposes, the significant thing is the accompanying restrictions on thought. There is an extraordinary uniformity about certain features of our behaviour; and a uniformity of thought, as well, so that small deviations from accustomed behaviour or appearances bulk large to us. We are almost incapable of thinking of real deviations. We commonly think of ourselves, we university and professional people, as giving little weight to fashions and appearances. We judge others by what they *are*, not how they look—or we think we do. But when I discuss this point with a group of undergraduates (who are also superior to appearances) I am accustomed to test them out, in a very mild way. I ask them some such question as this. "How well could you listen to a lecturer, whose words are wise and brilliant, let us say, but whose body is *really* deformed?"

Not the minor deformations you have been used to from infancy, such as a broken nose or a missing arm, but something really unusual in appearance?" When I describe a quite conceivable situation, of a man, perhaps a veteran, whose features have been obliterated, without hair, whose face is a mass of red scar tissue without the customary landmarks, with holes where ears and nose had been—when I ask such a question, I observe unease and embarrassment. When I ask, *à propos* of fashions, how they would respond to a lecturer who was *really* unfashionable, with feathers in his hair, a ring in his nose, and wearing women's clothing—once more, there is embarrassment and incredulity, as if such questions were somehow indecent.

There are extraordinary barriers to our thought in such matters. Not only do we have primitive taboo phenomena in this society—including the full-blown ambivalent form, not simply, as the texts would suggest, "moral prohibitions"—the taboos are taboos on thought as well as deed. Sex and religious taboos exist, of course; but not only these, as you can easily observe for yourself if you will just ask an audience like this why we need undertakers—why not simply carry the dead body out to the city garbage truck? Our religious views, after all, tell us that the body is an empty husk, of no significance. Perhaps you remember some correspondence in *Time*, some ten or twelve years ago, when there was meat rationing, and a correspondent reported that since Aunt Millie had fallen downstairs on a Thursday they had been able to have her over the weekend.

Perhaps I have horrified you; one could easily have been much more horrifying. But my purpose is not to flout convention, still less to ridicule it; I needed to go far enough to show you that these barriers to thought exist, since for the most part we are unaware of them. They are so effective that they efface themselves completely. As psychologists we must look at them, however, pleasing or not. Thompson and I have argued that they may be *essential*, in some form or other, to a sound society; if so, we must study their role, and the first requisite is to see them as they are.

So much for my first example. The second one of the two I have chosen to show what undeveloped regions there are in the continent of human thought—a continent, as you might say, teeming with wild life—my second example is the more engaging one of the hocus-pocus in which man finds so much attraction and delight.

Hocus-pocus is not, I think, an unduly flippant term; what I refer to is the attraction that comes from elaboration of the simple into the complex, the human love of paradox and magic, propaganda and poetry, puzzles and politics. My attention was first drawn to this long ago by my

father, a country doctor. I asked his help for a slight rhinitis, and he suggested the use of table salt in a glass of water, snuffed up my nose. A week or two later he asked me if it had helped. I had to admit that I hadn't got around to trying it, and he remarked that I was like most patients. When he was first in practice, he often recommended homely remedies, trying to save his patients money; and just as often the patient never did anything about it. But when he wrote out a prescription for *sodium chloride in aqua pura, T.I.D.*, and the patient had to go to the drugstore and pay for the prescription, or at least get billed for it, his advice acquired some practical value.

As psychologists we must be the last to scorn recognizing such means of influencing behaviour, and hocus-pocus is as comprehensive and intelligible a term as any for them. If I sound flippant, here or in what follows, I am not; I am only making use of a little hocus-pocus of my own brand. If we are seriously concerned with the theory of human behaviour, if we seriously propose to become as good as the politician or the fundamentalist preacher in predicting and controlling human behaviour, we must give serious concern to the empirical fact that man is attracted by what he does not understand, by the contradiction in terms, by the paradox and the preposterous statement. What else drives us perpetually to the *Why* of our existence—an unanswerable question, but one to which no man can help coming back again and again?

How else explain the full charm of poetry? Some of this, of course, is in its euphony; but even here, I think, some of the attraction lies in the fact that the poet, apparently without effort, achieves the impossible in combining rhythm and rhyme with some form of meaning—wholly analogous to the seemingly effortless grace of the ballet dancer, contradicting the observer's whole experience of his own movements in space. Neither ballet dancing nor poetry would hold attention to the degree they do if it offered us *only* rhythm and smoothness of movement or sound.

Consider the content of some of the poetry that has received universal admiration: the poetry that children are allowed to read, with no suggestion that they should take some of its statements with a grain of salt. There is Keats, for example, most persuasively urging that "beauty is truth, truth beauty," whereas if there were ever two things that one must keep separate in his mind, whether in political promises, women, or scientific theories, those things are Beauty and Truth.

Consider the compelling beauty of Swinburne's "When the hounds of spring are on winter's traces," and ask yourself whether the reference to "the lisp of leaves and the ripple of rain" is a fair description of getting caught in the bush in a spring rainstorm. Observe also, in the later

stanza, describing the onset of summer, the total failure to mention the mosquitoes.

Consider Browning's "Porphyria's Lover." The poet describes a girl who has stolen away from her husband or fiancé to meet her former lover secretly. According to Browning, this action gave evidence that the girl was, at that moment only, "perfectly pure and good." What is the sensible thing to do about a girl who is perfectly pure and good? Obviously, strangle her, and with her own hair too, making the whole account plausible and one of the most engaging in the whole of English poetry.

But here, on Canadian soil, Keats and Browning and Swinburne are in some degree foreign writers. For all our love of England, especially just now in this year of Her Majesty's coronation, I feel that we must recognize home products too; we Canadians too have something to contribute when it comes to illustrating the breadth and freedom of the human imagination. First, as is fitting, I give you an Ontario poet, James Gay, from the record that W. A. Deacon has provided in his book, *The Four Jameses*. For Gay's life you must consult Deacon, and I can give you here only enough to whet the appetite. Gay appointed himself *Poet Laureate of Canada*, and *Master of all Poets*. A sample of his work concerns Mary Queen of Scots—and as Deacon says, nowhere is his humanitarian concern more evident for people as people. Instead of royalty, and high tragedy, he stresses the womanly function with true poetic insight:

O Mary, Mary, Queen of Scot,  
Your needlework is not forgot;  
Three hundred years have passed, they say,  
Your beautiful piece of tapestry is still in the  
hands of Mrs. Thomas Dunn, of Nassagaway.

No student of comparative psychology should miss "The Elephant and the Flea." Further examples of his work are also provided by Gay in the letter he wrote to Tennyson on Longfellow's death, suggesting that the two remaining poets of the world should be friends, without petty jealousies, and also warning Tennyson to keep his son off the stage—Gay knew something about its dangers from having travelled with a sideshow to exhibit a two-headed colt.

I would not have you think, however, that Ontario stands alone, in the production of eccentric genius. Nova Scotia claims James D. Gillis, the author of that immortal work, *The Cape Breton Giant: A Truthful Memoir* (for this, as well as Gillis's poetry, see Deacon's book if you cannot find the originals). It was Gillis who said, by way of introducing himself to the reader, "I was twice to the United States; I do not say so

for the sake of boast." It was Gillis who settled on Spain and Columbus the final credit for discovering America. He pointed out that "it is of little interest to refer to the discovery of America by the Northmen. It was at best a slipshod affair, and resulted in songs which ordinary people . . . could not understand." It was Gillis, finally, who set a new standard in introductions: having carefully described his style, the authenticity of his writing and its moral value, he goes on to say, "Applicable quotations sparkle here and there, and where convenient are credited to their authors." Writers of Ph.D. theses, please note.

Well, I have perhaps gone far afield, but not too far. As psychologists, we have long been concerned with two aspects of human thought: (a) the formal solution of a formally stated problem; and (b) the clinical deviations of thought in gross mental illness. There is a much wider field. Freud, it is true, made valuable and stimulating applications of his ideas to the thought processes of everyday life; but, despite Mowrer, the psychoanalytical model has never been successful as a psychological model, and as psychologists we have refrained, on the whole, from discussing this larger field.

If, as I have proposed tonight, scientific understanding can be arrived at through relation of a limited but intelligible scheme or model or theory to phenomena that it can never fully explain, but to whose exploration it continually stimulates and guides us, we must ask of each new model what effect it has on the way in which we see the whole broad range of human behaviour, what guide it can offer us in the approach to reality. The model I have described to you tonight has, I think, opened new fields of development, if only by explicitly recognizing the existence of thought. It brings up insistently the question of the *motivation* of thought, the sources of pleasure and displeasure that bear no relation, as far as I can see, either to Freud's or to Hull's theory of the sources of human action. No theory, of course, comes near comprehending the phenomena of eccentric genius (of which Gay and Gillis are such excellent examples), of man's love of poetry and art, or of the social channelling and protecting of human thought. But as we work at our narrower theories we can at least keep that broader field in view, and hope that the gap between theory and life will become steadily smaller and smaller as our psychological knowledge develops.



## MORTALITY OF ALBINO RATS UNDER STRESS AS A FUNCTION OF EARLY HANDLING<sup>1</sup>

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STUDIES at the Wistar Institute of Anatomy and Biology in Philadelphia have indicated the marked effects of individual attention, including handling and petting, on growth and survival of the albino rat (2). For example, more than 75 per cent of gentled rats survive an operation for removal of the parathyroid, whereas less than 15 per cent of ungentled animals survive such an operation. The more albino rats are handled and petted, the better they seem to thrive in the laboratory situation. Other studies (1) suggest that handling and petting albino rats leads to a superior performance in discrimination learning, and, further, that rats so handled are more lively and vigorous, and show more exploratory behaviour, than comparable controls.

An unpublished pilot study by Weininger at Toronto suggests that an experimental group of rats, gentled for ten minutes a day for three weeks after weaning, will (a) gain more weight and (b) show less fearful behaviour in an open-field situation, than a control group of comparable rats not so gentled.

The work of Selye at Montreal (3) indicates that prolonged stress, such as cold or starvation, will lead to degenerative renal and cardiovascular changes in the rat and, if sufficiently prolonged, to the subsequent death of the animal.

The present study is an investigation of the effect of early handling on the resistance of the albino rat to severe stress in adulthood.

Two litters of five rats each, Wistar strain, were randomly sorted into three different groups at the age of 21 days, immediately following weaning, and were housed in individual cages. The mean weight of each group at this time was approximately the same, 37 grams (Figure 1).

The four male rats in the first group were gentled for ten minutes a day for the next ten days. Gentling consisted of holding the animal in the experimenter's left hand with the hand placed against the experimenter's chest, so that the animal was "cuddled" in the palm of the left hand, with the right thumb stroking the back of the animal from the head to the base of the tail, at the rate of approximately 50 strokes a minute. The animal was not held tightly and was allowed to move about in the experimenter's hand. During the gentling procedure the experimenter

<sup>1</sup>This work was supported by a grant from the National Research Council of Canada.

stood, for it was found in a previous study that if the experimenter remained seated the animal tended to climb on his lap. During the gentling it was noticed that all animals would arch their backs and make a clicking noise with their teeth. This has been interpreted by Greeman and Duhring as a sign of happiness or content (2).

The two female rats in the second group were picked up at the base of the neck, held for a moment, and released. They were not held tightly. This procedure was followed once a day for the same ten-day period.

During this time the third group, consisting of four male albinos, was not handled at all. In all respects except handling, treatment of the three groups was the same. Each animal had a 6" x 6" x 12" metal cage to himself and had a plentiful supply of Purina food pellets and water, in addition to small pieces of green vegetables supplied once a week.

From the end of this ten-day experimental period (by which time the

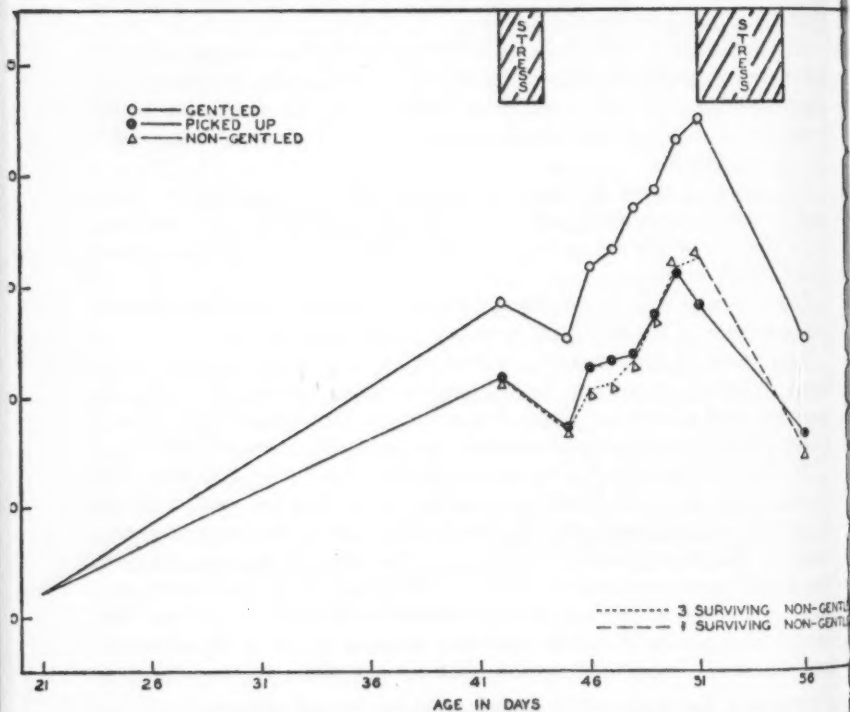


FIGURE 1. Average weight of rats in gentled, picked-up, and non-gentled groups.



animals were 32 days old) until they were picked up for weighing at 42 days of age, none of the rats in any group was handled by the experimenter.

When the rats were weighed on this last occasion, a wide divergence appeared between the weight of the gentled group and the weight of the other two groups, although, it will be recalled, all three groups started off at 21 days of age approximately the same weight. Now, however, the mean weight of the four animals in the gentled group was 115.5 grams, whereas the mean weight for the two animals in the picked-up group and the four in the non-gentled group was the same, 94.5 grams in each case.

Immediately after weighing, all three groups were subjected to intensive stress in the form of total food and water deprivation for 60 hours. To this stress was inadvertently added, through failure of the laboratory heating equipment, a drop of room temperature to 50° F. for the last ten hours of the deprivation period.

During the last period of combined cold and starvation, one of the four rats in the non-gentled group died. A superficial autopsy indicated enlarged adrenals, reddish brown in colour, and small bleeding points in the stomach lining, as well as possible degenerative changes in the kidney.

The nine surviving animals were weighed immediately after this initial stress period and once a day thereafter until the end of the experiment, as Figure 1 shows, but received no more handling than was necessary in this procedure.

Just before introduction of the second and final stress period, at the age of 51 days, mean weights were 165 grams for the gentled group, 114.5 for the picked-up group, and 127.6 for the three surviving animals in the non-gentled group.

All three groups were now subjected to another period of severe stress, this time consisting of total food and water deprivation for 120 hours. Two of the three surviving animals in the non-gentled groups succumbed during this period.

At the end of this time, the sole surviving non-gentled animal was observed to be very sluggish, manifesting little or no activity. Its eyes were dull in colour and half-closed, and its fur was mottled, with the hairs giving the appearance of standing on end. In contrast, the gentled and picked-up animals were lively, with rapid movements, and their eyes were still bright. While their fur was not mottled in appearance, the hairs did appear to be starting to stand on end. These observations were made independently of one another by two experimenters.

All the animals were fed and watered at the end of the 120-hour

deprivation period. The next day, however, approximately 20 hours later, the last surviving rat in the non-gentled group died.

A superficial examination of this animal, as well as of the three others from this group that had died during the experiment, showed enlarged adrenals of reddish brown appearance, bleeding points in the stomach lining, and possible deterioration of the kidneys.

Thus none of the six animals that had been handled by the experimenter, and all of the animals that had not been handled by him, died under stress in adulthood.

This difference in mortality rate cannot be accounted for exclusively in terms of body weight. Although it is true that just prior to the introduction of the final stress period the mean weight of the gentled group (165 grams) considerably exceeded that of the other two groups (114.5 grams for the picked-up group and 127.6 for the three surviving animals in the non-gentled group), nevertheless the fact remains that the mean weight of the picked-up animals, who survived the final stress situation, was considerably less, when they were weighed just preceding the final stress situation, than that of the remaining non-gentled animals, who did not survive.

This experiment suggests, but owing to the small number of animals involved does not conclusively demonstrate, that early handling in some undetermined manner influences the body metabolism of the albino rat in such a way as to increase its weight and increase its resistance to stress that is lethal for comparable rats not so handled.

There is an interesting parallel between the findings of the present study with respect to rats, and findings of Spitz on the effects of institutionalized care of human infants. Spitz found (4) a mortality rate of zero over a three and one-half year period for a total of 122 children in an institution where mothers or mother-substitutes were available for each child, and a rate of 37 per cent over a two-year period for a total of 91 children in an institution in every way medically comparable with the first, but where it was not possible to give individual care to the children.

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## REVERSALS IN THE PERCEPTION OF LISSAJOU FIGURES BY PSYCHOTICS<sup>1</sup>

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OF RECENT years the interest manifested in the study of the relationship between perception and personality has been so intense that current numbers of some psychological journals are almost entirely devoted to this topic. One should not overlook the fact, however, that there have been some few studies in the past which also studied this relationship. Thus, following the theories and experiments of McDougall (8), some authors have investigated the relationship between reversals of perspective and introversion-extroversion. McDougall thought that extroverts would have a smaller rate of reversals than introverts. His experiments substantiated his theory and were confirmed by Guilford and Braly (5), Guilford and Hunt (6), and George (4). But more recent and better controlled studies, using greater numbers of subjects, by Fredericksen and Guilford (3) and Fisichelli (2), found no evidence of relationship between these variables.

Using this type of investigation as a point of departure, Hunt and Guilford (7) reasoned that manic-depressives, who display extroversion to a pathological degree, should have a lesser reversal rate than dementia praecox patients (schizophrenics), who represent the opposite end of the introversion-extroversion scale. They actually did find such a difference, for their schizophrenic group yielded about four times as many reversals as their manic-depressive group, and this difference was highly significant. This finding, if verified, indicates an important relationship between personality and perception in psychotics.

Accordingly, it is the purpose of this investigation to compare the frequency of reversals of manic-depressives and schizophrenics, using a larger number of subjects and a slightly different technique from that of Hunt and Guilford.

### METHODOLOGY OF THE EXPERIMENT

#### *The Test Figure*

For their test figure Hunt and Guilford had used an eight-inch outline cube projected from eight feet away on a screen. The patients were told that this figure would reverse. The cube outline which they chose is possibly not the best one for such an investigation. A static figure is uninteresting and does not readily hold the attention of psychotic subjects

<sup>1</sup>This project was assisted by a Mental Health Grant from the Ontario Department of Health.

while they are observing it. Furthermore, these subjects are often highly suggestible and, unless the experimenter has some objective criteria for evaluating their actual perceptions, he is not sure whether they actually see what they report or are merely conforming to his suggestions.

Lissajou figures of the type reported by Philip and Fisichelli (9) and by Fisichelli (2) are apparently better suited for working with psychotics than are the book or cube figures used in previous investigations. When rotated slowly and continuously in one direction they suddenly seem to reverse their direction of rotation. So compelling is the perception of such reversals that the experimenter may easily note when they occur for the subjects by observing their eye movements, their indication by gestures of the direction of the movement, and their exclamations when such reversals occur. Since the Lissajou figures are dynamic rather than static like the book or cube outlines, they offer a much more interesting figure to the subjects and more readily hold their attention.

There is evidence that the Lissajou figures may be substituted with some confidence for the cube outline in this type of investigation. Fisichelli (2) found that the correlation between his static and dynamic figures for 144 subjects was .440, and Philip, in a preliminary investigation, found a correlation of .494 for 77 subjects between the book and the Lissajou figures.

As will be noted later, the reliability based on test-retest for psychotics is higher for the Lissajou figures ( $r = .84$ ) than it is for the cube outline ( $r = .54$ ). The first reliability value is based on the 110 subjects of the investigation reported here and the second reliability value was found by Hunt and Guilford (7) from 45 subjects.

For the above reasons a four-loop Lissajou figure was substituted in this investigation for the cube outline used by Hunt and Guilford.

### *Subjects*

The subjects for this investigation were all inmates of the Ontario Hospital at London, Ontario. They were selected for testing by one of the hospital staff who had instructions merely to keep the numbers of men and women and of manic-depressives and schizophrenics approximately equal. These subjects were listed, together with pertinent information from the files, but such information, with the exception of their names, was not given to the experimenter until after the test for each subject had been given and scored. Hence the experimenter was unaware of the diagnostic category of the subject he was testing until the scoring had been completed.

(a) *Psychiatric categories.* The diagnoses given were all based upon staff conferences. The number of subjects in each category is recorded in Table I.

TABLE I  
DIAGNOSTIC CATEGORIES OF THE SUBJECTS

Category	Men	Women	Total
Manic-Depressives			
Manic	9	3	12
Depressed	12	3	15
Mixed	5	14	19
	26	20	46
Schizophrenics			
Simple	2	1	3
Catatonic	6	7	13
Paranoid	16	18	34
Catatonic and Paranoid	2	6	8
Affective	3	3	6
	29	35	64
Totals	55	55	110

(b) *Age.* The subjects were not selected for age, which ranged between 20 and 78 years. The median age of the manic-depressives is considerably greater than that of the schizophrenics (57.0 as compared with 37.5), although the men and women in each category do not significantly differ in age. For the total group of men and women the median age of the men is about five years greater than that of the women (47.5 and 42.5), chiefly because there are proportionately more women in the schizophrenic group, which is the younger group.

#### *Procedure*

All subjects were tested individually at about the same time in the afternoon. They were brought into a slightly darkened room and comfortably seated 10 feet away from a screen. The experimenter, who sat slightly back and to the side of them, operated a projector which threw a picture 15 inches by 15 inches on the screen. When the picture was projected, instructions substantially as follows were given:

"On the screen you see an object like a wire paper basket which is turning round and round. Look at the front of the object and tell me in which way the front part is turning, right or left." (The movement was explained by gestures.) "Whenever the direction of the movement changes please tell me so at once, by saying 'right' or 'left' as the case may be."

It is obvious from the instructions that a passive attitude was required and that it was intimated that shifts in direction might be seen. When it was clear that the instructions were understood, the picture was

projected on the screen for exactly one minute, followed by a half-minute rest. Ten such trials were given at each sitting which lasted from 15 to 20 minutes per subject. During each minute trial the experimenter counted and recorded the number of reversals reported by the subject.

No subject had difficulty seeing the rotation, although not all subjects reported reversals of direction. When reversals were seen, the subject left no doubt about his perception, either by his exclamations of surprise, his gesturing (some followed the direction of the movement by gestures), or the shift in the direction of his regard. Nearly all subjects co-operated perfectly. Two subjects who were too ill to co-operate were excused, and their scores do not appear in the data.

### RESULTS

#### (a) *Distribution of Reversal Scores*

The total number of reversals reported by the subject was taken as his score for the test. Movements other than reversals were reported by 29 subjects (26 per cent). These perceptions were generally of a weaving, accordion, or rinsing type, and were always accompanied by fairly frequent reversals. Movements other than reversals were recorded but were not used in estimating the score. A considerable proportion of this psychotic population (38 of 110, or 34.6 per cent) perceived no reversals. Moreover, the distributions were considerably skewed. For these reasons it was decided to use  $\chi^2$ , corrected when required for continuity, to test significant differences, and to use medians rather than means as measures of central tendency. Since approximately half the subjects made scores less than five, a reversal of perspective score of five or more will be called "high," and one less than five will be called "low." These cut-off points for reversal scores will be used whenever significance is being tested.

#### (b) *Comparison of Schizophrenics with Manic Depressives in Frequency of Reversals*

The chief point of interest in this investigation is to determine whether the two main psychiatric categories score differently in their perception of reversals of Lissajou figures. The numbers of men and women in each category making high and low scores are presented in Table II, from which  $\chi^2$ 's were computed. From these data it is clear that the two broad diagnostic categories of manic-depressives and schizophrenics do not significantly differ in their perception of reversals of Lissajou figures. This result fails to confirm the findings of Hunt and Guilford.

It may be noted that these authors found that, as regards the number of their reversals, paranoids seemed to form a group intermediate between manic-depressives and the rest of the schizophrenics. Accord-

TABLE II

Category	Men			Women			Both sexes		
	Low	High	Both	Low	High	Both	Low	High	Both
Manic-depressives	18	8	26	9	11	20	27	19	46
Schizophrenics	15	14	29	11	24	35	26	38	64
Both	33	22	55	20	35	55	53	57	110
$\chi^2$	1.097			.571			2.814		
P	.29			.45			.09		

ingly, if, as in Table III, the paranoids are taken from the schizophrenic group and put in a separate group, a  $\chi^2$  of 6.58 is obtained. This value

TABLE III

Category	No. of cases		
	Low	High	Both
Manic-depressives	27	19	46
Schizophrenics less paranoids	10	20	30
Paranoids	16	18	34
Totals	53	57	110

of  $\chi^2$  reaches the acceptable level of significance ( $P = .05$ ), and the trend is in the direction of Hunt and Guilford's results, since the schizophrenics obtain a relatively greater number of reversals than do the manic-depressives.

Since the data on number of reversals of Lissajou figures, when dichotomized into high and low scores, seem to suggest a differentiation between psychiatric categories, it is of interest to examine whether this relationship holds for mean scores. In Table IV are listed the data on reversals from the various psychiatric categories.

TABLE IV

Diagnosis	N	Mean score	$\sigma$
Manic	12	14.50	16.56
Depressed	15	11.00	18.01
Mixed	19	8.42	12.72
Manic-depressives	46	10.85	11.96
Catatonic	13	4.39	4.95
Paranoid	34	15.09	18.67
Catatonic and paranoid	8	25.86	25.46
Affective	6	29.86	15.09
Simple	3	19.00	8.49
Schizophrenics	64	15.84	21.81



With the realization that the data are not normally distributed, and that hence the *t*-technique is not adequate to evaluate the significance of the differences between groups, nevertheless it was applied for the sake of comparison. The main groups, manic-depressives and schizophrenics, yielded a *t*-value of 1.39; and since a *t*-value of 1.982 is required for significance at the 5 per cent level, these two groups do not significantly differ in the number of reversals, confirming the findings from the application of the  $\chi^2$ .

When, following the suggestion of Hunt and Guilford, the paranoids are taken from the rest of the schizophrenics, the differences between the manic-depressives and this new group of schizophrenics less paranoids is not significant either, for the *t*-value is 1.57, and 1.992 is required for the 5 per cent level of significance.

An inspection of Table IV shows that the catatonics have the lowest mean reversal score of all the groups. If we remove the catatonics from the schizophrenic group and put them in with the manic-depressives, the *t*-value of the differences between these groups is now 2.72, which is significant, since the *t*-value for the 1 per cent level is 2.623. From these relatively sparse data it looks as if the catatonics, in their estimation of the number of reversals, fit more properly in the manic-depressive group than they do in the schizophrenic group. If we set up a four-fold table (Table V), using these new groups of diagnostic categories where the catatonics are removed from the schizophrenic group and are placed with the manic-depressives, we obtain a value of  $\chi^2 = 3.21$ , which is not significant at the 5 per cent level, since a value of *t* = 3.841 is required for significance at that level. However, two of the catatonics

TABLE V

	Low	High	Both
Manic-depressives and catatonics	33	26	59
Schizophrenics less catatonics	19	32	51
Totals	52	58	110

have reversal scores of 5, which is just at the cut-off point for the highs and the lows. When these two cases are assigned to the lows we get  $\chi^2 = 4.65$  which is significant at better than the 5 per cent level.

The shifting of the catatonics from the schizophrenics to the manic-depressive group might readily be questioned. A more acceptable mode of classification is to use as many categories as possible which give adequate numbers in the cells. To do so requires the grouping of three of the categories. We now get Table VI, from which  $\chi^2$  may be determined.



TABLE VI

Category	Low	High	Both
Manic	6	6	12
Depressed	10	5	15
Mixed	11	8	19
Catatronics	7	6	13
Paranoids	16	18	34
Remainder	3	14	17
Totals	53	57	110

A  $\chi^2$  determined from Table VI has a value of 9.42, which is less than that required for significance at the 5 per cent level ( $\chi^2 = 11.07$ ). However, if, as has been suggested, we shift the two scores of the catatronics which occur at the cut-off point from high to low, the  $\chi^2$  now becomes 11.72, which is significant at the 5 per cent level.

For the sake of comparison, if we make an analysis of variance from the scores of the subjects in the six categories in Table VII (bearing in mind the questionable validity of such a procedure owing to the skewness of the data), we obtain an F-value of 2.81, which is significant at about the 3 per cent level ( $P_{05} = 2.30$ ).

From the above analysis we might hazard the inference that the various diagnostic categories of psychotics, in this sample, show a slight trend to be differentiated according to their scores on the Lissajou figures.

(c) *Effect of Sex, Age, and Treatment on Reversal Score*

1. *Sex.* An attempt was made to determine whether certain variables might have influenced the reversal scores of this sample. The reversal scores made by men and women were divided into high and low and a four-fold table (Table VII) was compiled. The  $\chi^2$  from this table,

TABLE VII

Scores	Men	Women	Both
Low	33	20	53
High	22	35	57
Totals	55	55	110

corrected for continuity, is 3.66, which is at about the 6 per cent level of significance. While the women tended to make higher scores than did the men, the statistical significance of this result is doubtful, chiefly because there are proportionately more schizophrenics than manic-depressives among the women of this group, and the schizophrenics score higher than do the manic-depressives.

2. *Age.* On the assumption that age might be an important factor in differentiating the psychiatric categories on the basis of score, the data were analysed with reference to age. The two main psychotic groups were divided into "young" and "old," using a value close to the median as a cut-off point. For manic-depressives, subjects 57 years of age and over are taken as "old," and for schizophrenics, subjects 38 and over are taken as "old." The usual cut-off points were used for high and low in reversals (score of 5 and over for high). Table VIII shows the

TABLE VIII

	Manic-depressives				Schizophrenics		
	Low	High	Both		Low	High	Both
Young (56—)	18	7	25	(37—)	16	19	35
Old (57+)	9	12	21	(38+)	10	19	29
Both	27	19	46		26	38	64
$\chi^2 = 2.69$ $P = .09$				$\chi^2 = 0.44$ $P = .45$			

distributions and gives the  $\chi^2$ s. Obviously age is not a significant factor in the production of reversals in this group.

3. *Treatment.* Since many of the subjects of this investigation had received electric shock treatment (ECT), it seemed of interest to explore the effect of this treatment on the reversal score. The data

TABLE IX

	Low	High	Both
Some treatment with ECT	32	29	61
No treatment with ECT	19	30	49
Totals	51	59	110

are given in Table IX. The  $\chi^2$  calculated from this table is not significant ( $\chi^2 = 1.53$ ). There are some indications that those who have had no ECT treatment tend to score high.

The data were also analysed to determine whether recent treatment was different in its effect on the number of reversals from remote

TABLE X

	Low	High	Both
Recent ECT	16	12	28
Remote ECT	16	17	33
Totals	32	29	61

treatment. Recent treatment is here defined as occurring within 24 hours of the time of the test for reversals. The data are given in Table X. It is plain that there is no significant difference between these groups.

A similar four-fold table (Table XI) was prepared to show the

effect of treatment upon the reversals of manic-depressives and of schizophrenics who had ECT. The resultant  $\chi^2$ , which equals .05, is significant.

TABLE XI

	Low	High	Both
Manic-depressives with ECT	17	12	29
Schizophrenics with ECT	15	17	32
Totals	32	29	61

(d) *Reversals and Rorschach Criteria of Rigidity*

Since the relationship between reversals of Lissajou figures and psychiatric categories is slight and there is no relationship between age or treatment and reversals, it seemed of interest to determine whether some personality factor, independent of psychiatric classification, might show relationship with the perception of reversals. Accordingly, it was hypothesized that the tendency to see few or no reversals in Lissajou figures might be indicative of perceptual rigidity. Unfortunately, the concept of rigidity varies widely among different authors and the criteria for its evaluation are often vague. Since the Rorschach is essentially a perceptual test, it was felt that the criteria of rigidity used in that test might be utilized for comparing perceptual rigidity and the reversal test.

Some dozen or so measures of rigidity based upon Rorschach determinants are given in the literature. They have been summarized by Cowen and Thompson (1), and most of them were used as measures of rigidity in testing our hypothesis. In condensed form they are as follows: for the rigid group as compared with the non-rigid group, fewer R, M, C, and O responses, higher F%, F+%, A% and more P responses.

Of the 110 subjects in the present investigation, 36 subjects, 16 men and 20 women, had been given the Rorschach by staff members of the hospital. On the basis of the median scores made by this group, a cut-off point between a high and low group was obtained for each determinant.

TABLE XII  
FREQUENCIES OF HIGH AND LOW SCORES ON THE RORSCHACH

Reversal score		Rorschach test									
	Determinant	R	M	C	F%	F+%	A%	P	O	R	
	Median score	15	1	2	50	84	50	4	1	1	
High group (N=20)	High group	11	12	8	7	15	9	12	4	5	
	Low group	9	8	12	13	5	11	8	16	15	
Low group (N=16)	High group	8	7	7	11	5	7	6	4	5	
	Low group	8	9	9	5	11	9	10	12	11	

The frequencies with which these high and low scores were obtained from subjects assigned to the high and low groups on the basis of their reversal score are listed in Table XII.

The relationship between each Rorschach measure of rigidity and the frequency of reversals was determined by the  $\chi^2$  technique, taking into account the correlation between the two sets of scores. All  $\chi^2$ 's were insignificant, save one, and in this case the relationship was in the opposite direction to that expected. The hypothesis was, therefore, rejected. If rigidity is a factor in the perception of reversals, at least it is not the type of rigidity manifested on the Rorschach.

*(e) Comparison of Normals and Psychotics on the Reversal Test*

Prior to the main investigation here reported, a preliminary investigation was carried out on 77 delinquent boys in groups of from 9 to 31. Apart from the technique of group testing the material, instructions, and method were the same in all respects as those used for the main experiment. The purpose of this preliminary experiment was to determine the feasibility of this technique with persons of average or low intelligence, to compare the perception of static (book) with dynamic (Lissajou) figures, and to estimate test-retest reliability. By using the data of the preliminary experiment with delinquent boys, and those of Fisi-chelli (2) with university women, a comparison was made between these two non-psychotic groups and the psychotics of the main experiment. The data are presented in Table XIII. The reliability of the scores of both normal groups was based upon tests given within a few days of each other and was of the order of .70. In order to determine the reliability for the psychotic population, the first five trials of the test were correlated with the last five trials, and a value of .84 was obtained. This value is somewhat questionable owing to the skewness of the data, caused chiefly by the fact that 38 subjects saw no reversals at all. When their scores were eliminated, the reliability for the remaining 72 subjects is still high ( $r = .79$ ). In general, the test-retest reliability for both groups is adequate.

TABLE XIII  
COMPARISON OF NORMALS AND OF PSYCHOTICS

Group	University women	Delinquent boys	Psychotics
Number of subjects	144	77	110
Age range	18-27	12-16	20-78
Mode of testing	Individual	Group	Individual
Number of tests on			
Lissajou figures	4	5	10
Median score per minute	5.20	7.56	0.63
Number of zero scores	11	4	38
Test-retest reliability	.72	.70	.84 (.79)

It can be seen from Table XIII that normals have a higher reversal rate per minute than psychotics, and that there are considerably fewer zero scores in the normal group. Both these differences are highly significant.

#### SUMMARY AND CONCLUSIONS

As a check on the findings of Hunt and Guilford, who stated that manic-depressives see significantly fewer reversals of Lissajou figures than do schizophrenics, a study was made of a group of 110 psychotics. There is a slight tendency for manic-depressives to yield lower scores than schizophrenics but it is of borderline statistical significance.

There is stronger statistical evidence that when these two broad psychiatric categories are broken up into more specific diagnostic categories they can be differentiated on the basis of this test.

Age, sex, electric shock treatment (ECT), and Rorschach determinants of rigidity are not related to reversal score.

Characteristic of psychotics, as compared with normals, is their tendency to see fewer reversals per minute. In fact, a considerably greater proportion of them (34.6 per cent), as compared with normals (6.8 per cent), see no reversals at all.

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## SYSTEMIC MODELS FOR SOCIAL GROUPS<sup>1</sup>

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THIS PAPER is an attempt at reformulation of the old problem of the individual and the group in systemic terms, that is, viewing the group as an organized system.

### A FRUITLESS DICHOTOMY

It is not intended here to go into the history of the problem, except to point out that most approaches have been based on an antithetical division between the individual and the group, in which primacy is claimed for one or the other. The crucial difficulties of this dichotomous approach to the problem are, in my opinion: first, its mechanistic or vitalistic orientation, and secondly, its assumption of a one-way causal relationship.

The mechanistic view that a whole is identical with the sum total of its parts is no longer tenable in the light of Gestalt psychology. But when we say, "The whole is more than the sum of its parts," we may mean two different things, depending on whether we are assuming the internality or externality of relationship. Humphrey (3) has acknowledged Koehler's contribution in applying to physical systems the philosophical doctrine of internal relationships, which assumes that the elements of a whole are fundamentally modified by their membership in the whole and their relationship with other members. The behaviour of an isolated element is therefore different from its behaviour within the context of the whole, so that no mere summation of the isolated elements can account for the properties of the whole.

The second meaning of the part-whole relationship implies the externality of relationship, that is, that the elements are fundamentally unaltered by entering into a relation. In this view, the whole is regarded as more than the sum of its parts only in the sense that it consists of the parts plus their relationships. The *Gestaltqualität* of von Ehrenfels and the Graz school is an example, and has been criticized by Koffka (4) as follows: "The Graz school, following up the ideas of von Ehrenfels, found that no sensation theory could explain shapes, and therefore they added to the concept of sensation, which they left untouched, the concept of a higher mental function, 'production,' and of its product, the *Gestaltqualität* (4, p. 559). The concept of *Gestaltqualität* is, he says, vitalistic, and for that reason unacceptable to Gestalt psychology.

<sup>1</sup>Paper presented at the 12th Annual Meeting of the Canadian Psychological Association, at Queen's University, Kingston, Ontario, May 1953. The author expresses her deep appreciation to Professors J. D. Ketchum and D. C. Williams for their stimulation and encouragement of her work on this problem.

The doctrine of Gestalt psychology proper is that the whole is the sum neither of its parts nor of the parts plus their relationships, but that its properties are non-additive. As Lewin (5) said, the whole is not "more than" the sum of its parts but has different properties. This point of view accepts the internality of relationship which is basic to Gestalt doctrine. Hence the properties of the whole can never be accounted for within the mechanistic or vitalistic frame of reference.

The second crucial difficulty of the dichotomous approach is the one-way causal relationship. No matter in what terms we define "group"—interaction, interdependence, participation, or relationship—as long as we look at the problem in terms of a one-way causal relationship, no solution will be found. Either the individual or the group will be regarded as affecting, influencing, or determining the other, and thus the antithesis between the two will be perpetuated.

#### THE SYSTEMIC APPROACH

A hopeful new approach to the problem, one which avoids both the elementarism of mechanistic or vitalistic association and the pitfall of a one-way causal relationship, is its reformulation in systemic terms. In regarding the social group as an organized system we assume that the members of a group are mutually related and interdependent, that the behaviour of any member is a function of his relationships with other members and with the group as a whole, and that his behaviour in turn affects the behaviour of all other members. With such assumptions we abandon the mechanistic view that the behaviour of the group can be predicted from the behaviour of its members as individuals, the vitalistic view that the behaviour of the members is determined by the group superstructure, and also the notion of one-way causal relationships and the consequent dichotomy between individual and group.

The concept of system in its current connotations originated in mathematics and the physical sciences; as Whitehead has said, the idea of an organized whole, or system, existing in an environment, is a fundamental concept essential to scientific theory (3). This notion is now being freely applied in the social sciences, and particularly to group phenomena, but before accepting the concept of system we need to examine it in its various definitions, and note whether its fundamental assumptions and principles are congruent with social-psychological phenomena.

#### PHYSICAL AND BIOLOGICAL SYSTEMS

We should first note certain important distinctions between physical and biological systems, pointed out by Humphrey and von Bertalanffy.



According to Humphrey's helpful analysis (3), the main characteristics of a *physical system* are as follows:

1. A "system" may be defined as a complex of logically distinct elements which are considered to be connected by a special set of interrelations; a complex in which the elements are so interrelated that they form a unity.

2. In this definition a distinction is inherent between "inside" and "outside"; the interrelated elements are thought of as inside the complex, and everything else as outside it. The relations of the elements to objects external to the system are not part of the interrelations connecting the constituent parts of the system.

3. Theoretically all physical systems are partial or open systems except the universe itself, which is the only total or closed system; but practically there are many physical systems which may best be regarded as closed systems by logical abstraction. According to Poincaré, "there is not in nature any system perfectly isolated, perfectly removed from all external action; but there are systems almost isolated" (3, p. 20).

4. A system must exhibit some sort of constancy. In a closed system such constancy is given by the assumption of the persistence in the system of identical material elements and relations. When it has been disturbed, therefore, there will ultimately be a return to a constant state, unless work has meanwhile been done on the system from an outside source; for the constant state implies the exclusion of every source of increased or diminished energy. This is the fundamental assumption of the physical sciences known as the second law of thermodynamics.

However, certain phenomena in physics and chemistry, such as the Le Chatelier and buffer systems, may be better understood in terms of an open or partial system; in this case the identity of the system is represented by persistence of its pattern of organization in spite of change in its material elements. This foreshadows the biological system, which, as we shall see, has similar characteristics.

5. There are at least certain physical systems, open or closed, the elements of which are fundamentally modified by their membership in the system, and these are synonymous with the physical Gestalt, in which relations are necessarily internal.

As regards the *biological or vital system*, Humphrey (3) states that it is essentially an open system, and points out two important characteristics:

1. Since an organism is not isolated from its environment but is in continuous relationship with it through stimulus-response relations and the exchange of material and energy, the vital system is always part of a more inclusive one, which comprises both the organism and



its environment. It is thus a partial system, though with a specific organization of its own.

2. The vital system maintains its total pattern, in spite of gradual and continuous alteration of its material components; in other words, it is characterized by constancy-in-flux, in contrast to the stable constitution of inorganic forms.

According to Humphrey, the principle of equilibrium, which is a fundamental postulate of the physical sciences, and is derived from closed systems, cannot be directly applied to a vital system. Since its equilibrium is disturbed at every moment by the action of the environment, it can never in reality reach a constant state. Instead, the vital system passes through a succession of different equilibria, each involving a change in level, and thus slightly modifying the total pattern. Hence, a biological equilibrium is conditioned both by the internal organization of the vital system and also by the character of the environment.

This is the point made by Bertalanffy (1), who advocates an "open system" model for biological and psychological organisms. The fact that the organism is a system, he says, is no reason for assuming that it should follow the thermodynamic laws of equilibrium, since it is definitely an open system, which continually gives up matter to the outer world and takes in matter from it. While Bertalanffy and Humphrey agree on this point, they differ in their interpretations of equilibrium.

Humphrey believes that, in the conservative equilibrium which the organism attains through its mechanisms of self-maintenance, the vital system obeys the physical laws of thermodynamics, but in a special way, determined by the peculiar organization of the system in question. He makes it clear, however, that this does not mean reducing biological problems to physical ones. Bertalanffy, on the other hand, puts forward a new principle, "the steady state in open systems," on the basis of his assumption that at each level of the hierarchy of sciences new properties emerge and therefore new laws also.

By his principle of "the steady state in open systems" Bertalanffy means that a vital system may maintain itself in a steady state of metabolism, instead of entering as quickly as possible a state of stable equilibrium, like physico-chemical systems. "The organism as a whole," he says, "is never in true equilibrium, and the relatively slow processes of metabolism lead only to a steady state, maintained at a constant distance from true equilibrium by a continuous inflow and outflow, building up and breaking down of the component materials" (1, p. 125). This principle, he claims, has the revolutionary consequence that, in the transition of an open system to a steady state, there may be a decrease in entropy and a spontaneous transition to a state of higher

heterogeneity and complexity. This is in opposition to the second law of thermodynamics which states that the entropy of a closed system can never decrease; in so far as there is any change in quality of energy, it is merely the general tendency toward a degradation of energy, that is, final equilibrium. Equilibrium in a closed system is based on reversible reactions, whereas the steady state in an open system is based on irreversible processes; a true equilibrium is incapable of doing work, whereas a steady state is capable of continuing to do work; the final state of the former depends upon the initial conditions, whereas that of the latter is equifinal, dependent not on the initial conditions but on the ratio between inflow and outflow.

These writers make it clear that the laws appropriate to closed systems apply only in their own sphere and are not directly applicable to open systems. I would like to suggest, however, that we cannot regard inorganic and organic systems simply as closed and open systems, respectively; the line is not so sharp. Some physical systems may best be regarded as open systems, as in the case of the Le Chatelier and buffer phenomena mentioned above; on the other hand, there are, as we shall see, social systems which behave very much like closed systems. Whether a system is to be treated as open or closed must depend on the nature and degree of its relationship with its environment. If, by logical abstraction, a system is treated as a closed one, it is assumed that it will tend to return to a state of homogeneity, regularity, and maximal simplicity, as would be implied by the thermodynamic laws of equilibrium or Koehler's "pregnancy theory" derived from them. If, on the other hand, it is regarded as an open system, the assumption will be that its equilibrium will resemble Bertalanffy's "steady state," and that there may be a spontaneous transition towards a state of higher heterogeneity and complexity.

#### SOCIAL GROUPS AS SYSTEMS

Now, in the light of what we have learned about physical and biological systems, let us attempt to describe the characteristics of social groups in systemic terms.

1. A social group, as a system, is conceived to be a complex of two or more individuals connected by a special set of interdependent relationships. The behaviour of each is conditioned and modified by communication with other members, and it in turn affects the behaviour of all other members. Each member is related, not only to other members, but also to the group as a whole and its products. For instance, in the process of group interaction, norms of behaviour emerge which

require the members to subordinate their individual goals and interests to those common to the group.

2. A social group, as a system, obeys the doctrine of internal relationship. In other words, the behaviour of its members is altered and modified by the relationships involved in membership. Group behaviour is therefore not wholly accounted for in terms of the pre-existing characteristics of its individual members, that is, a group is best regarded as a social Gestalt.

3. A social group, as a system, displays an inherent differentiation between "ingroup" and "outgroup" attitudes. The development of the ingroup-outgroup delineation can be better understood by reference to the concepts of open and closed systems. Under specific conditions, a social group may be regarded as a closed system, though of course only by logical abstraction. A good example is the case of the so-called "suicide bombing squadrons" in Japan. These young fliers were trained in a small tight group, almost completely isolated from the outside world. As a troop, of course, they were still part of the larger system and aware of it; but there was actually little communication with the outside world, so that such a group closely approximated a closed system. As the ingroup-outgroup delineation becomes sharper, more rigid, and impermeable in such a group, bias and hostility towards outsiders grow tremendously in contrast with the ingroup solidarity, as Homans (2) has pointed out. Solidarity under these circumstances is so greatly strengthened by the stabilization of each member's self-perception as part of the group that extreme self-sacrifice can readily take place. Certain small, isolated, primitive societies might similarly be treated as closed systems.

In general, however, groups are characterized better as open than as closed systems. As Sherif says, "groups, especially today, are not closed systems" (6, p. 122). The behaviour of members is regulated and determined by their membership of the group, but they also refer themselves psychologically to other groups and regulate their behaviour accordingly. The psychological ingroup-outgroup delineation is therefore more permeable and flexible, and there are recognized limits to the amount of individual sacrifice which the group can demand.

4. A social group, as a system, exhibits a certain constancy. To groups which approximate a closed system, as mentioned before, the law of equilibrium seems applicable. The tendency towards homogeneity derived from the law of equilibrium is represented by an increase of the cognitive, affective, and conative identification of the self with the other members. Of course, in such a group there are also differentiations in the organization of the ingroup structure. But these differences tend to become highly institutionalized and thus are quite

compatible with the maintenance of strong solidarity or identification between the members.

The group process in a dynamic open system, on the other hand, may be better characterized by Bertalanffy's principle of "the steady state in open systems." Instead of relative isolation from the external world, there is continuous and close contact; instead of a stable, self-contained equilibrium, we have an endless series of adaptations to the problems of maintaining itself and performing its tasks in the environment. Instead of a progression towards relative homogeneity in its members, the meeting of these successive problems may give rise to new differentiations, new patterns of integration, and thus a higher order of group organization. In other words, for most groups in our society, Bertalanffy's concept of an open system, maintaining a "steady state," seems the most appropriate model.

#### CONCLUSIONS

To sum up, then, we consider that regarding the social group as a system can be of great value in social theory, since it enables us to escape the individual-group dichotomy and to account for such aspects of group behaviour as mutually dependent interaction and group control over the individual in terms of the group process itself. At the same time, we must be careful not to assume that the laws of human behaviour are identical with, or even analogous to the laws established for physical systems, which can generally be treated as closed. The suggestions of Humphrey and Bertalanffy as to the appropriate system-models for biological and psychological systems do not solve all the difficulties, but they should prevent us from making unwarranted assumptions about human group behaviour and suggest directions in which profitable research might be planned.

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# AN APPARATUS FOR THE ANALYTICAL STUDY OF HANDWRITING MOVEMENTS

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THE MEASUREMENT of motor activities involved in the drawing of simple patterns, tapping, and handwriting, under stressful and normal conditions, has been shown to be a valuable and significant motor test. An attempt has been made to find a reliable method which allows rapid and uncomplicated follow-up testing of large groups, similar to the method of the EEG or ECG.

Preliminary experiments with apparatus based on mechanical-pneumatic principles have not proven satisfactory. The results obtained with these methods were somewhat similar in terms of reliability to those reported by Ruesch and Finesinger(12), Pedersen(11), Gemelli(5) and Steinwachs(13, 14). Hence it was thought necessary to develop an electronic apparatus similar to those described recently by Wenger(15), Kroner(6) and von Bracken, Pungs, and Riedel(2). The apparatus to be described in this article (Figure 1) was designed independently<sup>1</sup> from those of the above-mentioned authors and has features not included in their designs.

## APPARATUS

During the past three years more than 1,000 subjects under normal and various stress situations have been tested with this *Electroscriptograph*(ESG), and it has proven to be an uncomplicated, reliable, and satisfactory instrument from various points of view(9, 10).

By means of this apparatus it is now possible to take three electrical measurements at the same time: (a) the point-pressure (pressure of the writing instrument against the writing surface), (b) the grip-pressure (pressure of all fingers grasping the writing instrument), and (c) the difference between point- and grip-pressure ( $a-b$ ). These measurements are registered on photographic paper by means of a three-channel oscillograph.<sup>2</sup> The speed of film movement can be varied between 1.3 and 80 cm. per sec.

The apparatus consists of: 1. a portable box (500/350/70 mm.) containing the electrical circuits, and having a writing plate (185/35 mm.) flush with the top surface for measuring the point-pressure; 2. a special pen- or pencil-holder (diam. 12 mm.) equipped with a transducer for measuring grip-pressure; 3. a 3-channel oscillograph which records the variation of the pressure on photographic paper.

<sup>1</sup>The apparatus was constructed by A. Muehlfeld (9).

<sup>2</sup>A multi-channel ink-writer could also be used.

The basic method for converting pressure into an electrical output is the same for point- and grip-pressure; application of pressure in each case causes an increase in the capacity of a condenser which is part of a resonant circuit (see Figure 2). This circuit is normally tuned to a frequency close to that of a small oscillator, and when the capacity increases, the circuit is thrown further off tune in approximately direct proportion to the pressure; this voltage, after rectification and one stage of D.C. amplification, is applied to the galvanometer of the oscillograph. Voltages of opposite polarity derived from the two main amplifiers are applied to a third amplifier to give the difference between grip- and point-pressure which is recorded by the third channel of the oscillograph.

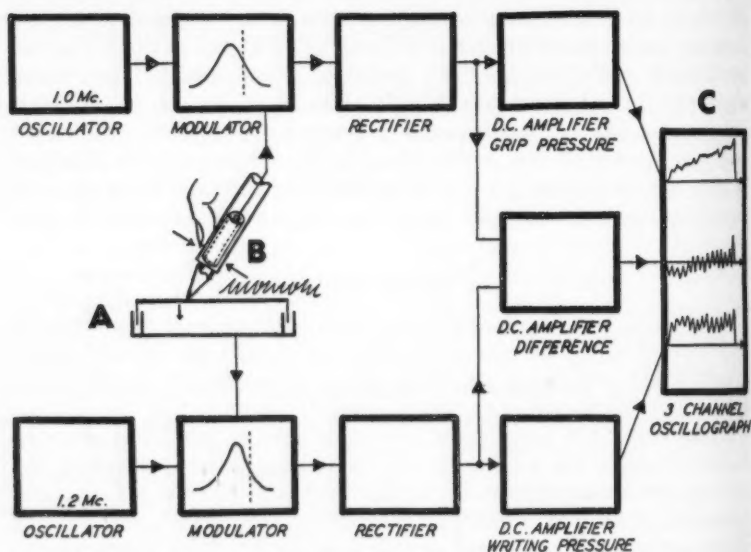


FIGURE 2. Block diagram of ESG circuit. A = the writing plate ("diving" condenser); B = the penholder (circular condenser); C = recording apparatus.

### Writing Plate

The writing plate is a grounded metal sheet, supported at both ends by springs. The sides of the plate are bent downwards at right angles to the surface and pass between two parallel insulated plates to form a "diving" condenser. The paper is placed on the writing plate which becomes depressed slightly during writing or drawing (0.2 mm. deflection per 500 gm. pressure—natural frequency higher than 130 cycles), thus increasing the capacity of the condenser. The maximum variations in

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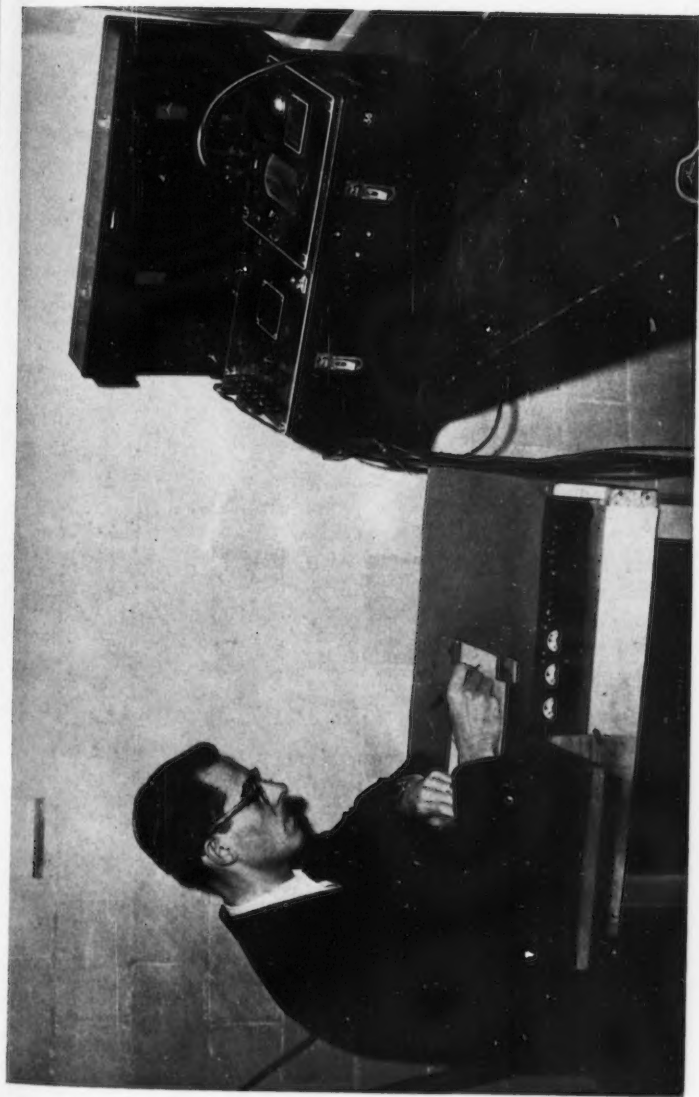
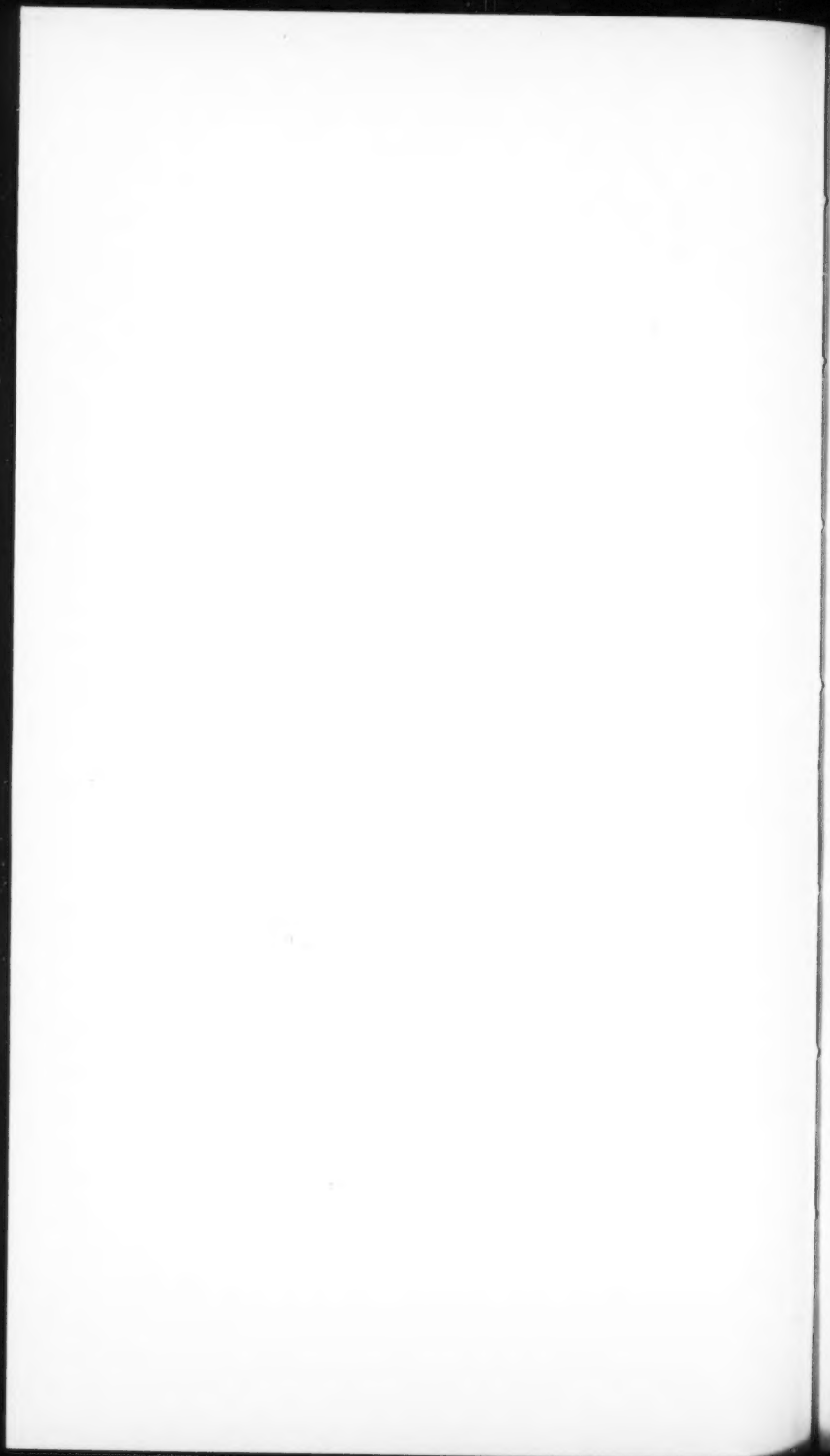


FIGURE 1. The *Electroscriptograph* (ESG) in use. The ESG is mounted on the table on the left.  
The oscillograph is on the bench to the right.





capacitance that might have resulted from applying the same pressure to different regions of the diving condenser (writing plate) were reduced sufficiently to cause no misleading artifacts in oscillographic registration by the following means: the sides of the writing plate were bent down at right angles, thereby insuring greater rigidity of the surface than could be obtained by using a simple plane surface; the contour of the lower edge of the bent sides was altered until there was never more than a 7 per cent (400 gm. pressure) difference between the capacitance reading at the centre and at any corner; and finally, further possibilities of distortion were eliminated by fixing two springs, made of spring steel, to the narrow ends of the diving part, rather than using a four-spiral spring arrangement (Figure 2). The natural frequency of the writing plate, which can be varied by a mechanical device, is usually maintained at 130 cycles. This is sufficiently high to eliminate any effects on the record which might be attributable to the writing plate itself and not to the pressure applied to it. Since the whole writing plate moves, regardless of where pressure is applied to it, point-pressure calibration was carried out by putting varying standard weights on the plate. By the Method of Least Squares<sup>(7)</sup> this point-pressure arrangement was shown to be linear at different degrees of sensitivity (medium sensitivity:  $y = 0.005x + 0.13177$ ).<sup>3</sup>

The comparatively large size of the writing plate thus made possible gives the instrument an advantage over instruments designed for a similar purpose but having smaller plates. Since the vertical height of normal handwriting is between 9 and 13 mm., and the average length of a line is about 170 mm., it is possible for the subject to write two lines before the position of the paper has to be changed. The value of the records obtained is enhanced by this fact, for it has been shown that, from the interpretive standpoint, the full characteristics of grip-pressure are not obtained until a sequence of meaningful words has been written (9).

#### *Penholder*

The penholder is also fitted with a pressure-sensitive condenser; the part normally grasped by the fingers (45 mm.) consists of a plastic tube surrounded by a copper foil, around which (and insulated from it by a layer of cambric) is another concentric tube of copper foil. These two copper foils are the two plates of a condenser, the capacity of which increases as the grip of the fingers squeezes the thin outer tube nearer to the rigid inner tube. The upper part of the central plastic tube contains

<sup>3</sup> $y$  = recorded amplitude (29.2 mm. for 700 gm. point-pressure);  $x$  = pressure in gm. (25-700 gm.).

the coil which, together with the condenser, makes up the resonant circuit. The connection to the rest of the apparatus is through a thin (2 mm.) flexible lead (Figure 1), coupled by a low impedance loop, so that movements of the lead during the writing or drawing do not affect the resonant frequency of the tuned circuit.<sup>4</sup>

Since the methods of converting point-pressure (writing plate—"diving" condenser) and grip-pressure (circled condenser) into an electrical output differ, the methods of calibration also differ. The pressure exerted on the circled condenser varies with the size of the fingers and the particular style of grip used by the subject, so a simple weighting procedure using standard weights could not be used for calibrating grip-pressure. The circled condenser of the penholder was tested by means of a pneumatic cuff connected to a manometer (pressure = mm./Hg.). The linearity of the instrument was demonstrated by the Method of Least Squares (medium sensitivity:  $y = 0.279x + 7.6480$ ).

Some of our preliminary experiments with pneumatic-mechanical apparatus (8), and other recent results under stress and normal conditions, have shown that the qualitative relationships between grip- and point-pressure (parallelism of pressure changes, formation of grip-pressure units, plateau-shaped curves, splintered wave formation, etc.) are of greater value than an exact quantitative grip-pressure definition in grams over the whole range of the continuum. As a result, until better means are found for interpreting electronic units in terms of grip-pressure (in gms.) the instrument reading can only be interpreted as being produced by ranges of grip-pressure, namely, *very low*, *low*, *medium*, *high*, *very high*. The use of such categories overcomes the otherwise impossible problem of dealing with the pen's being held at different angles relative to the hand and to the paper by different subjects, and the resulting variations of the component of a given grip-pressure which acts at right angles to the circled condenser. Furthermore, since the subject's individual style of holding the pen is not affected, and since the circled condenser itself has statistically stable characteristics, this method would appear to be superior to the alternative method (used by Pedersen (11) and Wenger (15) ) of recording the pressure of only one finger.

The difference between point- and grip-pressure is recorded as a third curve by electrically subtracting the measurement of point-pressure

<sup>4</sup>It is probable that either strain-gauges, or preferably electronic transducer-tubes, could advantageously be substituted for the condensers as electro-mechanical transducers. (At this point we wish to acknowledge the valuable help received from Mr. P. M. Millner, McGill University, Montreal for suggestions regarding the technical part of this paper.)

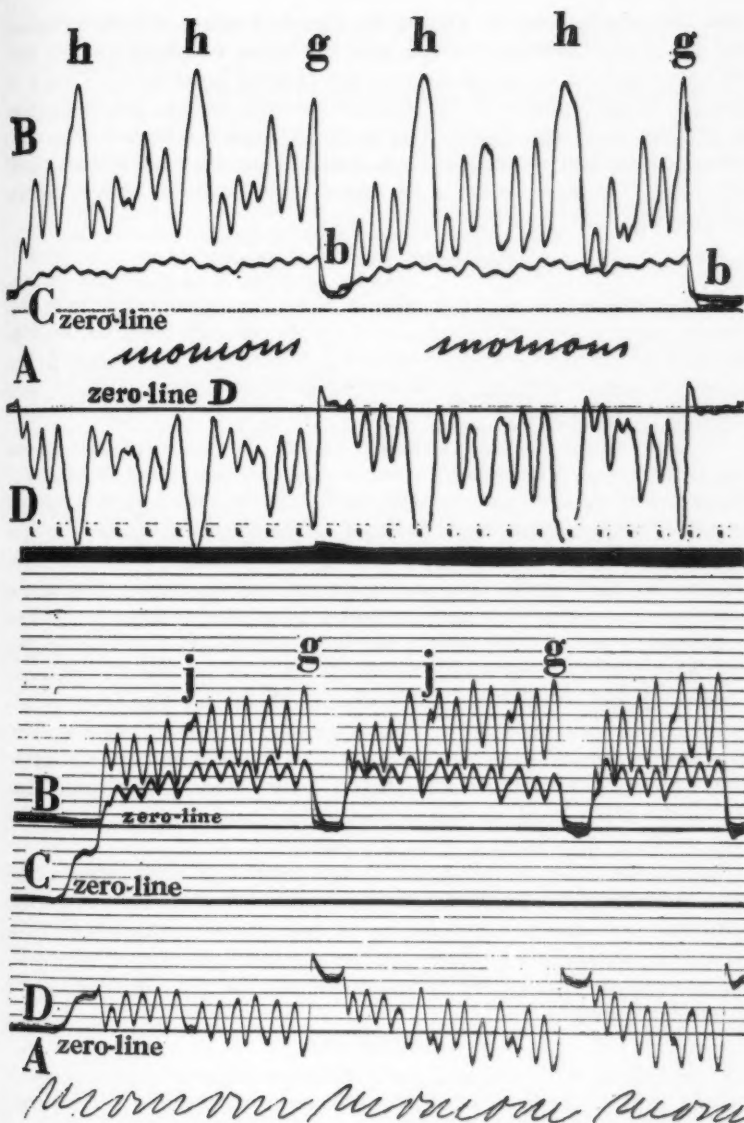


FIGURE 3. Sample records obtained from the ESG. Record for  $S_1$  at top; that of  $S_2$  at bottom. A = written test words "momom"; B = point-pressure; C = grip-pressure; D = pressure difference.

from that of grip-pressure. Thus, if the electrical values of both pressures are equal, the difference is zero and the curve corresponding to this difference will fall on or cut the zero-line (Figure 3, curve D). Thus it is possible to get a picture of the relations between the two functions; this is of theoretical and practical interest for correct interpretation (9). Direct immediate control of each system (point-, grip-pressure, and difference) is made possible by three corresponding visible meters (Figure 1).

#### PROCEDURE AND EXAMPLES

S sits comfortably at a small table in which the apparatus is built; the writing paper is fixed by two clips to the plate and only the table and the sheet of paper can be seen (Figure 1). Recording begins after S has been familiarized with the given writing situation, writing material, and items to be performed.

Figure 3 shows samples of records of two S's (speed of film movement: 1.3 cm./sec.). Both S's were writing the test word "momom".<sup>5</sup> There are obvious differences between S<sub>1</sub> and S<sub>2</sub> with regard to point-pressure, grip-pressure, and pressure difference. The point-pressure (curve B) of S<sub>1</sub> shows irregular changes in intensity, excessive high peaks (h), final peaks (g), and large differences during the whole writing process (A). That of S<sub>2</sub> is more regular, final peaks (g) are less pronounced, and there are only a few splintered waves (j). The writing time for each "momom" is shorter for S<sub>2</sub> than for S<sub>1</sub>. The grip-pressure (curve C) of S<sub>1</sub> is relatively low compared to the corresponding point-pressure (the difference curve D in such cases lies beneath the zero-line, D); the rhythmical pressure-changes are very small and do not show any clear relation to the corresponding point-pressure. The record for S<sub>2</sub>, on the other hand, reveals a greater tendency for grip-pressure to vary in the same manner as does point-pressure.

Within the limits of this paper it was possible to show only a few characteristics of the curves recorded. Additional features have also been quantitatively treated in the scoring of such records. Analysis of ESG records in terms of the psychological factors involved offers a wide field for speculation, and special consideration will be given to this aspect in later publications.

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<sup>5</sup>The test word "momom" was introduced by Enke (4) and was later used by Carmena (3), von Bracken (1), Steinwachs (13, 14) and Luthe (9, 10).

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## BOOK REVIEWS

*A General Selection from the Works of Sigmund Freud.* Edited by JOHN RICKMAN. Psycho-analytical Epitomes No. 1. Toronto: Clarke, Irwin & Co. Ltd., 1953. Pp. x, 332, \$2.50.

THIS volume is the first of a projected series of four books relating to psycho-analysis. As its title suggests, it is intended to epitomize the contributions of Freud to the theory of psycho-analysis. In the words of the editor it is:

An exposition of psycho-analytical theories regarding the interplay of the forces of instinct in the human mind, the mechanisms employed in this interplay and the principles governing the reduction of "tension" or strain in the mind. It deals also with topography of the mental apparatus, particularly in respect to those parts of the mind that are concerned with instinct-impulses, with consciousness, with conscience and with the regulating or executive mental functions.

To summarize the development of a writer's thought by selecting judiciously from his works is never an easy task and will probably always offend the purist. With Freud, whose theory (to his credit) grew and changed with the years, it is particularly difficult. Fortunately, few people could be better qualified than John Rickman, and, in the opinion of the reviewer, he accomplishes his purpose admirably.

He has included fifteen selections (standard translations), representing, as he points out, less than one-twentieth of Freud's works. Each one has been trimmed so as to omit less relevant details, while retaining the main current of thought. Starting with the Clark University Lectures in 1909 on the *Origin and Development of Psycho-analysis*, the reader can follow the growth of Freud's thinking on the Unconscious, Repression, the Instincts, Libido, Narcissism, Ego, Id, and Super-Ego, and other corner-stones of psycho-analytic theory, up to 1932.

The main quality such a book should possess is, of course, representativeness. In statistical language, as a sample, it should represent the universe from which it is drawn. In the opinion of the reviewer, it has this quality. Rickman has made only two really noticeable omissions, namely, *The Interpretation of Dreams*, and *The Psycho-pathology of Everyday Life*. At least parts of these might be considered as having importance in the development of Freudian theory. On the other hand, there seems little necessity for including an extract entitled by the editor "A General Aetiological Formula," from the paper "A Reply to Criticisms of the Anxiety-Neurosis." Again, the amount of space devoted to *Group Psychology and the Analysis of the Ego* seems excessive, although Rickman attempts to justify it in his preface.



Finally, the chronological sequence of presentation might well have been adhered to more closely. The selection from *Negation*, for example, although it was published in 1925, occurs in the book before other works published in 1912 and 1915. There are two other similar instances.

But these are minor criticisms. On the whole, the book should be an excellent addition to any psycho-analytic library and serve usefully in undergraduate or graduate courses in systematic psychology. The appended list of English translations of Freud and the combined glossary-index are particularly valuable.

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*Psychology and Art of the Blind.* By G. RÉVÉSZ. Translated from the German by H. A. WOLFF. Toronto: Longmans, Green & Co., 1952. Pp. xiii, 338. \$8.40.

THIS book is divided into two parts. The first part is an attempt to establish a theory of Haptics—that is, of the impressions conveyed by the tactile and kinematic senses. The second part deals with the aesthetic experience and the sculptural works of the blind.

The author points out that our knowledge of the haptic world is meagre and that he is offering a first treatise on tactile aesthetics. He is well aware of the fact that the work is in its preliminary stages, but his investigations have raised a number of important problems from the point of view of both aesthetics and psychology. The analysis of the creative activity of blind sculptors has provided data which will, in his opinion, influence general aesthetics and the history of art.

This book will be of special interest to those who are intrigued by the character of Haptics and by its importance in the field of perception of both the seeing and the blind.

LEOLA E. NEAL

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*Your Deaf Child.* By H. R. MYKLEBUST. Toronto: Ryerson Press, 1950. Pp. xv, 132. \$3.25.

THE task of understanding and training a deaf child, as Dr. Myklebust states, is not an easy one. However, in his opinion, parents can do a great deal for a deaf child during his preschool years if they accept as a challenge the problem which he presents and if they believe that he is in many ways like any normal child. Often parents need encouragement and special guidance if they are to give the child the training he needs.

This book, *Your Deaf Child*, is written as a guide for parents who have a child with a loss of hearing which is so great that it has prevented him from learning to talk as he should. Although Dr. Myklebust is a technical expert in the field he puts his ideas in simple, direct language and he writes in a helpful way for all parents of deaf children.

The author deals with the nature, causes, and types of deafness, parents' attitudes (acceptance, over-protection, wishful attitude, indifference), the deaf child's needs (need for consistency in training, being a part of the family, success, activity, independence, health, expression), problems of training, (eating, eliminating, discipline, temper tantrums, fears), communication (use of aids, speech, going to school), and finally what to expect from your child.

A feature worthy of special mention is the directory of schools for the deaf and hard of hearing in the United States and Canada. The author also includes a list of organizations which are interested in providing information to parents of deaf and hard-of-hearing children, and a list of books and periodicals which are especially helpful.

As mentioned above, this book is expected to be of greatest value to parents. It should also be of much use to professional people who come in contact with preschool children who are deaf.

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*Fundamentals of Social Psychology.* By EUGENE L. HARTLEY and RUTH E. HARTLEY. Toronto: McClelland and Stewart (New York: Alfred A. Knopf), 1952. Pp. xxxv, 740. \$6.35.

THIS book is designed to serve as a text for students of social psychology, as well as a valuable source of supplementary reading for those interested in the psychology of advertising and journalism, genetic psychology, and personality theory. An effective integration of key findings in anthropology, sociology, psychology, and psychiatry has been accomplished, and the authors have been successful in carrying out their avowed intention of staying within the framework of social psychology, conceived as the study of the basic principles governing human interaction.

The volume is divided into three parts. The first two parts, dealing with semantic problems and the importance of communication in socialization, and with the genesis of the individual as a socialized being, are rather theoretical in nature. The third section in itself presents a course in social psychology from the standpoint of human experience. A large amount of experimental material has been collected, arranged,

and interrelated, and the authors are to be congratulated on the comprehensive nature of the completed task.

Perhaps in a presentation which is intended to be as comprehensive as this, a discussion of the findings from studies of hypnosis and suggestion would be desirable, but this is probably the only important omission. In general, while the organization of a large amount of material has been effected, the style of the book may leave something to be desired. It is repetitive, rather than straightforward and concise, and in this respect reflects the involved style of Gardner Murphy who, incidentally, has obviously influenced the authors' thinking in no small way. The multiplicity of headings also may lead to confusion rather than to clarification, since the reader's attention is often diverted from important issues and central problems to a consideration of subsidiary aspects of these problems. Finally, many of the studies reported are of the questionnaire variety. While this fact in itself is in no way detrimental, the rather uncritical acceptance of such data may be misleading.

There are a number of special features which ought not to be overlooked in reviewing this book. In particular, the magnificent scope of the work makes for an integrated and cautious approach to problems in the determination and origin of group behaviour. Especially noteworthy, perhaps, is the inclusion of material from those psychoanalysts who have been able to foresee the relevance of social psychology to any adequate approach to psychotherapy. In this regard, the writings of authors such as Horney, Sullivan, Fromm, and Lindner have been used as effective sources of dynamic material. The thought-provoking nature of Murphy's approach to the learning of social attitudes in terms of canalization is echoed throughout the book. Due importance is attached to the problem of status and status determination in considering the motivation for social behaviour. Finally, a dynamic conclusion for the author's task has been found in the review of the derivation and possible alteration of ethnic attitudes.

In general, this book should find its place among the standard reference works in the psychological library of any university. As a source of reference material it is invaluable; as a demonstration of effective organization within a defined framework it is also particularly noteworthy.

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## TO OUR CONTRIBUTORS

A CHANGE of editors provides an appropriate moment for saying something to those who make the *Journal* possible—our valued contributors. The following points are not a formal statement of policy; they are merely remarks on matters which should probably be mentioned from time to time in the interests of mutual understanding.

1. The *Journal* is glad to receive articles of psychological interest for publication. "Psychological interest" is at present broadly interpreted to include all recognized areas of psychological research, theory, and application, as well as the teaching of psychology and the relation of psychology to other disciplines.

2. While our primary aim is to serve Canadian psychologists, no qualifications of citizenship, residence, or membership in the CPA are required of contributors.

3. Manuscripts should be sent to The Editor, Canadian Journal of Psychology, 100 St. George Street, Toronto 5, Ontario. Selection of articles for publication is made by the Editor and a panel of consultants, on the basis of intrinsic merit, general interest, and suitability in other respects.

4. The maximum brevity consistent with clearness is a recognized aim in all scientific writing. In a journal containing only 48 small pages it is particularly important, and excessive length may militate against acceptance of an article. While exceptions occur, it is our belief that most researches can be adequately described, and most points of view developed, in from 2,500 to 4,000 words—many in less than 2,500. Condensation usually improves an article, rather than otherwise.

5. The *Journal* has neither a full-time editor nor a paid secretary, and the handling of editorial work and correspondence on a spare-time basis can be very burdensome. The amount of both could be greatly reduced if all contributors would prepare their articles carefully for publication before submitting them.

Such preparation requires skill, but the skill is one which anyone who writes for publication can readily acquire. A number of useful guides are available, the most recent and appropriate being the *Publication Manual of the American Psychological Association* (Supplement to the *Psychological Bulletin*, Vol. 49, No. 4, Part 2; July, 1952). The manual may be obtained by anyone from the APA office, 1333 16th Street N.W., Washington 5, D.C.; price \$1.00.

The nine sections of the Manual, with one exception, are all relevant to our journal, and study of them will be most profitable to all interested in writing. The one important exception, in our case, is Section 7, on *References*; for the present we ask our contributors to consult recent issues of this journal and follow the style of references there employed.

6. Under Dr. Long's editorship the *Canadian Journal of Psychology* has reached a position of which we may all be proud. It is widely read in the United States as well as in Canada, and authors report requests for offprints from every important American centre. With the steady growth of psychology in Canada the *Journal* should continue to improve; attention to some of the points mentioned above will greatly help the editorial staff to do their part of the job.

THE EDITOR

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